OUR ASTRONOMICAL COLUMN.

SOLAR INVESTIGATIONS.—*Astronomy and Astro-Physics* for February contains a short note by Prof. Hale, to the effect that photographs have been taken at Kenwood Observatory, showing the H and K lines reversed in regions widely distributed over the sun's disk. These regions closely resemble faculæ in appearance.

At the meeting of the Paris Academy on March 7, Prof. Tacchini communicated a paper on the distribution in latitude of the solar phenomena observed at the Royal Observatory of the Roman College during the second half of last year. Prominences have been more frequent in the northern hemisphere than in the southern, although in the preceding half-year, and in 1889 and 1890, they were more frequently observed in the southern solar hemisphere. The zones of maximum frequency occurred between latitudes 40° and 60°. Faculæ also have been most numerous north of the equator, and the zone of maximum frequency of these phenomena appears to be between latitudes 10° and 30°. Spots have been most abundant north of the equator, with a maximum frequency in the same zones as faculæ.

In the *Comptes rendus* containing Prof. Tacchini's results, occurs also a note by J. Fényi, on a remarkable prominence observed at Kalocsa, on February 19, as the recent large spot-group was passing over the sun's limb.

NEW DOUBLE STAR, 26 AURIGÆ.—In a communication to the *Astronomical Journal*, No. 256, Mr. S. W. Burnham records the discovery that 26 Aurigæ is a close double star, made up of two nearly equal components. His measures of position-angle and distance for 1892·0 are 344°·4 and 0″·15; and of magnitudes, 5·6 and 6·0. The distance very probably never exceeds a quarter of a second, or the duplicity of the star would have been noticed by many observers of the distant companion discovered by Herschel in 1783.

ROTATION OF JUPITER.—Writing in the March number of the *Observatory*, Mr. Denning notes that his observations of one of the chief dark spots in Jupiter's north temperate belt, for the period from August 21 to November 3 (180 rotations), gave the mean period of rotation 9h. 49m. 36·9s. Observations of the red spot, from August 7 to February 2 (432 rotations), indicate a mean period of 9h. 55m. 42·2s. The value obtained during the opposition of 1890 was 9h. 55m. 40·2s., so that the motion of rotation of the red spot would appear to have slackened by two seconds. Since the period of rotation derived from this spot is now 6m. 5s. longer than that given by the dark spots on the north temperate belt, the latter revolves around Jupiter, relatively to the former, in 40½ days.

THE NEW STAR IN AURIGA.—In No. 3078 of the *Astronomische Nachrichten* are recorded three communications relative to this Nova, two of which refer to its position, while the third deals with its spectrum. The observations of the last-named were made at the Astro-physical Observatory in Héreny, Ungarn, by Herrn Eugen and Gothard, previous to February 15. On the 10th and 13th of the same month the following lines were observed:—

Bright lines.	February 10. Wave-lengths in micro-millimetres.	February 13. Wave-lengths in micro-millimetres.
I.	654·2	—
II.	532·2	530·3
III.	513·2	516·8
IV.	501·9	501·9
V.	492·3	492·3
VI.	486·6	486·2
VII.	439·0	—
End of spectrum	412·0	—

In the first series of observations, the authors give suggestions for some of the lines, and for comparison's sake we have added Mr. Lockyer's also, both of which are included in the following table:—

Wave-lengths.	Origin.		Lockyer.
532·2	Chromosphere line	(531·7)	— (531·3)
501·9	Nebula-line	(500·5)	Mg (500·6)
486·6	Hydrogen line Hβ		H (486·1)
439·0	,, ,, Hγ		— —

In the second series comparisons were made with Geissler's tubes, and the following measurements were made:—

	Geissler Tubes.	Nova.
IV.	500·6	501·9
V.	492·6	492·3
VI.	486·2	486·2

THE LICK SPECTROSCOPE.—The February number of *Astronomy and Astro Physics* contains an excellent plate, taken from a photograph by Mr. Barnard, of the spectroscope on the great 36-inch refractor of the Lick Observatory. In the description of the instrument it is stated that the spectroscope itself is no less than 130 pounds in weight, while the two brass rods which connect it to the telescope form an extra addition of 75 to 80 pounds. Accompanying the plate, which shows the general arrangement of its parts, is a plan of the instrument which is completely described in the text. Many ingenious ideas have been displayed in the completion of the instrument as regards accessories, such as that of the lighting up of the pointers and production of the comparison-spark. Owing to the great focal length of the telescope, only 1·06 inch of the full aperture of the spectroscope can be used, but when it is dismounted, it rests on a truck, and its full aperture, 1·50 inch, is then available for laboratory work.

A BRIGHT COMET.—A circular from the Royal Observatory, Edinburgh, communicated by Dr. Ralph Copeland, and dated March 11, contains the following information of the appearance of a bright comet:—

Dr. L. Swift discovered a bright comet at Rochester, N.Y., at 16h. 50·1m. local mean time, on March 6, its place then being R.A. 18h. 59m., South Declination 31° 20′. It was moving eastwards.

The exact place of the comet was observed at the Royal Observatory, Cape of Good Hope, on the 8th inst. to be:—

Cape of Good Hope Mean Time	16h.	58m.	36s.
Right Ascension	19h.	2m.	27·8s.
South Declination	30°	2′	54″

Astronomische Nachrichten, No. 3079, also contains some information with regard to this comet. A telegram from Boston contained the following: "Comet Swift was observed by Barnard, March 8·0399 G.M.T.; R.A. app. = 285° 51′ 20″, Polar Distance = 120° 32′ 53″. Comet is visible to the naked eye."

Another telegram from Capetown read: "Comet was observed March 9·6024 G.M.T.; Right Ascension = 287° 45′ 50″, P.D. app. = 119° 16′ 12″. R.A. March 8 read, 286° 36′ 57″ instead of 285° 36′ 57″."

Prof. H. Kreutz in the same number gives the following elements and ephemeris:—

Elements.

T = 1892 March 26·8545 Berlin M.T

$$\left.\begin{array}{rl} \omega = & 9^\circ\ 57'{\cdot}21 \\ ☊ = & 239\ 33{\cdot}76 \\ i = & 33\ 38{\cdot}44 \end{array}\right\}\ \text{M. Eq. } 1892{\cdot}0$$

$\log. q = 0{\cdot}02208.$

Ephemeris for 12h. Berlin M.T.

1892.		α h.	m.	s.	δ °	′
March	9	19	10	25	− 29	22·9
	13		29	13	25	57·8
	17		46	57	22	22·0
	21	20	3	41	18	39·0
	25		19	26	14	52·6
	29		34	17	11	5·9
April	2	20	48	19	− 7	21·9

CALCULATION OF TRAJECTORIES OF ELONGATED PROJECTILES.

AS the correct determination of the law of resistance of the air to the motion of elongated projectiles is a matter of considerable national importance, I hope you will allow me to offer a few concluding remarks explanatory of the present state of the case. Some results of my first systematic experiments were published in 1868, and General Mayevski, after applying a few tests of his own, adopted my results in his "Traité de Balistique Extérieure," 1872. Siacci made use of these results in his ballistic tables (1881). But Mayevski appears to have recently become a disciple of Krupp, from the diagram in Nature, August 28, 1890 (p. 411), where the dotted line (1) represents roughly the resistance of the air to ogival-headed projectiles given in my "Final Report," 1880; line (2) represents the law of resistance deduced from these results by Major Ingalls, of the United States Artillery, which is similar to the law deduced by me (Nature, April 29, 1886, p. 605); and line (3) represents the results Mayevski professes to have deduced from Krupp's Meppen experiments. My law of resistance has been very closely followed throughout by Mayevski, as is evident from the diagram above referred to, which is suggestive of a free use of the parallel ruler. The main object of these proceedings seems to have been to persuade the world, and the Americans especially, that Krupp guns are far superior to English guns, regard being had to the initial steadiness imparted by them to their projectiles. But this claim is unworthy of notice so far as it depends upon the Meppen experiments with chronoscopes, the patent defects of some of which were pointed out in Nature, April 29, 1886 (p. 606). If, however, Government consider this matter worthy of investigation, there are simple practical methods of determining the comparative steadiness of projectiles fired from two or more guns.

At present, my concern is with English guns only, and I wish to point out, as briefly as possible, (1) that my results obtained from English guns are quite correct; (2) that the coefficients of resistance for each round are expressed by such a short unit of time that they are made to appear more irregular than they are in reality, while the variation in their value is just what experiment leads us to expect; and (3) that when my *mean* coefficients are *fairly* used to calculate results of *good* experiments made with *recent* English guns, in *calm* weather, the agreement between calculation and experiment is perfectly satisfactory.

My chronometric arrangements were made with a view to guard against the errors of remaining magnetism, which is the chief source of error in the measurement of extremely short intervals of time by the help of electro-magnetism. All the time-records were made by one electro-magnet, whose galvanic current was interrupted once a second by the swing of a half-seconds clock pendulum; and all the screen records were made by another electro-magnet, whose galvanic current was being rapidly interrupted by a self-acting contact-breaker, till the pull of the lanyard turned off the contact-breaker, and then fired the gun, after which the shot momentarily interrupted the galvanic current as it passed each of ten or more equi-distant screens. Also care was taken to reduce the strength of the galvanic currents, so as to leave each electro-magnet only just sufficient power for the performance of its appointed work. Under these circumstances it may be safely assumed that, if there were any errors arising from remaining magnetism, in either clock or screen records, they would be *constant* in each case, and therefore they would have no injurious effect on the result obtained. The records on a 4-inch cylinder were read off by a vernier to the 1/3000 of its circumference; but as the scale of time was in general only 9 or 10 inches to the second, it may be concluded that the records were read off to the 1/2000 of a second at least.

The accuracy of the time and of the screen records was tested by differencing, when slight adjustments were applied to render the first and second order of differences regular to an *additional place of decimals*. The following is a list of the adjustments so applied in seven successive rounds, 146–152, which are fair samples of those applied in the other rounds (1867–68). They are expressed in decimals of the unit read off by the vernier, or of the 1/2000 of a second. In round 258 an example is given of the correction of an occasional erroneous record at screen 6:—

Round	146.	147.	148.	149.	150.	151.	152.	258.
Screen								
1	+0·3	+0·2	−0·2	0·0	+0·2	0·0	0·0	− 0·1
2	−0·5	−0·1	+0·5	0·0	0·0	+0·2	+0·6	+ 0·1
3	−0·3	−0·2	−0·1	0·0	−0·7	−0·3	−0·1	+ 0·2
4	−0·1	−0·1	−1·0	0·0	+0·1	+0·4	−0·1	− 0·6
5	+0·1	+0·2	−0·2	0·0	+1·4	+0·3	+0·7	− 0·1
6	+0·3	−0·2	+0·3	0·0	+0·3	+0·5	+0·3	+32·9
7	−0·4	+1·6	+0·4	0·0	−0·2	0·0	−0·4	+ 0·6
8	−0·1	+0·7	−0·7	−1·0	−0·1		+0·7	+ 0·2
9	+0·2	0·0	+0·1	0·0	+0·6		−0·4	− 0·1
10	−1·5	−0·4	−0·2	0·0	−0·1		+0·2	− 0·1

Here is conclusive evidence of the perfect trustworthiness of the observations made, such as no other ballistic experiments have afforded to my knowledge. When the readings of the screen records required only such slight adjustments as those above indicated, there could be no reasonable doubt about the perfect accuracy of the experiment, and the round was accordingly adopted as good in all cases, unless there was some known disturbing cause, as when the bronze gun expanded, or where the gas check left the shot, &c.

Although the records are read off only to the 1/2000 of a second, we are able to express the coefficients of resistance with much greater exactness through the employment of a long range (1350 feet) where the only absolute errors in time possible are at the two extremities of the range, and the accuracy of each of these readings is tested by the differencing. Supposing the retarding force of the air, acting upon an ogival-headed projectile moving in the direction of its axis with a velocity v f.s., to be expressed by $-2bv^3$, the values of $2000bw/d^2$ corresponding to all velocities from 900 to 1700 f.s. were found by experiment in 1867–68, where w denotes the weight of the projectile in pounds, and d its diameter in inches. Corresponding to a velocity of 1200 f.s., the mean value of $2000bw/d^2$ was found to be 0·0001089. But to avoid the use of so many decimal places, K was subsequently employed to denote $(1000)^3 2bw/d^2 = (1000)^3 w\Delta^2 t/d^2 l^2 = 108{\cdot}9$, where $\Delta^2 t$ is the second difference of the times at which the shot passed successive equi-distant screens l feet apart, with a velocity 1200 f.s. more or less, and, in the case of the solid 5-inch shot it equals 0″·00124. From this it appears that for the specified shot the time by which the unit of K is expressed is 0″·0000112 in $\Delta^2 t$. Considering the shortness of this unit of time, it seems very natural that some variations should have been found in the experimental values of K for any specified velocity, derived as they have been from both *hollow* and *solid* projectiles fired with *various charges* from 3, 5, 7, and 9-inch guns. If we turn to actual experiment, it is plain that the coefficients of resistance for any given velocity cannot practically retain a constant value for all rounds. For do not we frequently read that shot are "noisy," or "unsteady" in their flight? There was much unsteadiness in the Jubilee rounds; and Captain May, R.N., in speaking of experiments with *recent* guns, remarked, "the range of 500 yards is selected, because at this range shell which *start unsteadily* will have steadied (that is if they ever do so), &c." It is, therefore, quite natural that exact experiment should afford evidence of this unsteadiness.

It now remains to test the value of my mean results by making use of them to calculate the ranges and times of flight of projectiles for comparison with the results of experiments made with *recent* guns. In 1879 some range tables of the 6·3-inch howitzer were forwarded to me to show that my coefficients for *low* velocities did *not* give satisfactory results. As the muzzle velocities in these tables were 332, 507, 628, 697, 740, and 751 f.s., and the elevations varied from 5° to 40°, the trajectories were much curved, so that my general tables were not applicable in these cases. But when the ranges and times of flight were properly calculated by Bernoulli's method, experiment and calculation were found to agree remarkably well. In the same way numerous German range tables (Krupp guns?) were calculated for muzzle velocities varying from 380 to 774 f.s., which gave very satisfactory results in general. Although there was no allowance for jump or vertical drift in these calculations for *low* muzzle velocities, the calculated often exceeded slightly the experimental ranges, showing that my resistances were perhaps a little too low. The results of each of these comparisons—32 English and 82 German—will be found in my "Final Report," 1880, pp. 45–47. For specimens of the best and worst results of each kind, see Nature, April 29, 1886 (p. 606). Now Mayevski proposes to reduce these coefficients of resistance, already rather too low, by 20 per cent. more! (Ingalls, pp. 29, 36).

In consequence of the Krupp scare, the authorities desired to have the accuracy of my results tested by practice on a *long* range, with a *recent* gun, and for this purpose *they selected* the 4-inch B.L. gun. Careful experiments were subsequently carried out with this gun (1887), which showed that my coefficients of resistance were perfectly satisfactory. But there was no real necessity for any special experiments to be made with this gun, as its own range table was afterwards found to be abundantly sufficient for the purpose of testing my results. By calculating trajectories carefully by Bernoulli's method, and then recalculating by the general tables the time of flight over the range already obtained, and also the striking velocity, it is found that the general tables may be used for elevations of the 4-inch gun as high as 15°, or even more, with a muzzle velocity of 1900 f.s. In this way the merest tyro may test my coefficients for his own satisfaction by calculating the times of flight over ranges of two or three miles given in any good range table for a high muzzle velocity. The following are the results of such testing, using the full extent of the range table of the 4-inch B.L. gun, chosen by the authorities. Muzzle velocity, 1900 f.s.; weight of ogival-headed shot (two diameters), 25 pounds :—

Range	2000.	3000.	4000.	5000.	6000.	7500 yds.
Horizontal muzzle vel.	1898·5	1895·0	1888·0	1875·0	1853·5	1796·8 f.s.
Horizontal striking vel.	1124·9	955·4	843·4	751·2	667·6	556·5 f.s.
Exp. time	4″·21	7″·20	10″·49	14″·3	18″·4	26″·2
Calc. time	4·17	7·10	10·48	14·30	18 66	26·43
Difference	−0·04	−0·10	−0·01	0·00	+0·26	+0·23

Here, as before, the calculated time is rather too short for velocities 1900 to 751 f.s. And if we allowed for a slight diminution of the density of the air for the higher elevations, as we ought to do, the calculated would throughout fall very slightly short of the experimental times of flight. Thus it is clear that my coefficients of resistance give perfectly satisfactory results when fairly tested by *recent* guns, chosen by Government, for velocities 330 to 751 f.s., and from 751 to 1900 f.s., or from 330 to 1900 f.s.

In the same way we may use the model range table, carefully prepared for the 12-inch B.L. gun by Captain May, R.N., for a muzzle velocity 1892 f.s., and weight of shot 714 pounds (Proc. R.A. Inst., 1886, p. 356) :—

Range	1000.	2000.	3000.	4000 yards.
Experimental time ...	1″·66	3″·47	5″·44	7″·61
Calculated time ...	1·654	3·457	5·428	7·591
Difference	−0·006	−0·013	−0·012	−0·019

Here, again, the calculated times of flight, being a trifle too short, show that my coefficients of resistance are very slightly too low.

When coefficients tested in this manner give calculated times of flight accurately over ranges gradually increasing up to two or three miles, those coefficients must be correct for all practical purposes, and they will give correctly the striking velocity and time of flight for any other reasonable distance from the gun.

The tables of "Mayevski nach Siacci," printed by Krupp, 1883, may be used to calculate the times of flight of the shot fired from the 4-inch gun as above :—

Range ...	2000.	3000.	4000.	5000.	6000.	7500 yds.
Experimental time	4″·21	7″·20	10″·49	14″·3	18″·4	26″·2
Calculated time ...	4·10	6·91	10·11	13·69	17·67	24·58
Difference ...	−0·11	−0·29	−0·38	−0·61	−0·73	−1·62

From this it is evident that the reduction of my coefficients proposed by Mayevski on the strength of Krupp's experiments is uncalled for.

Again, it has been urged that my resistances ought to be reduced in order to adapt them to *recent* guns, which, it is assumed, impart an increased degree of steadiness to their projectiles. But that assumption requires proof. After most carefully testing the admirable range tables of the 4-inch and 12-inch B.L. guns, I have failed to find any indication whatever of increased steadiness in their projectiles. Besides, Admiral Robert A. E. Scott wrote to the *Morning Post* (November 9, 1889), condemning the system of rifling the 110-ton gun, which he blamed for causing the projectiles to "issue from their guns with a *very unsteady motion*." He then went on to notice the large number of unsteady shot fired from the 9·2-inch gun in 1888. I would also remind my critics that my coefficients of resistance for velocities 1000 to 1700 f.s. were derived from experiments made in 1867–68, while all those for velocities less than 1000 f.s., and greater than 1700 f.s., were derived from experiments in 1878–80, carried out with some of the newest and best guns of the time. As conclusive evidence of the excellence of the 3, 5, and 7-inch guns used in the early experiments, reference may be made to the fact that, from the results of the experiments of 1867–68, I was able to deduce the Newtonian law of resistance for velocities 1350 to 1700 f.s. (Proc. R.A. Inst., 1871); and using the mean of the eight numerical coefficients there given for velocities 1350, 1400, . . . 1700 f.s., the numerical value of k will be found to be 143·9.

In 1879 experiments were made with a *new* Armstrong 6-inch B.L. gun, with velocities 1700 to 2250 f.s. (Reports, &c., Part ii., 48); and again, in 1880, further experiments were carried out with a *new* Armstrong 8-inch B.L. gun, with velocities 2250 to 2800 f.s. (Final Report, 56). Combining these three sets of experiments, Major Ingalls found that the Newtonian law of resistance held good for velocities 1330 to 2800 f.s., where $k = 142·1$ (Ext. Bal., 36). I also deduced the same law for velocities 1300 to 2800 f.s., where $k = 141·5$ (NATURE, 1886, p. 606). And lastly, after a thorough revision of the reduction of every round, I finally adopted the same law for velocities above 1300 f.s., where $k = 141·2$.

Hence it appears that the early experiments of 1867–68 were so accurate that they gave a correct law of resistance for velocities 1350 to 1700 f.s., which has since been found to hold good for velocities 1300 to 2800 f.s.; and they also gave the coefficient $k = 143·9$ (with *studded* shot) sufficiently accurate for all practical purposes up to a velocity 2800 f.s. This is conclusive evidence of the steadiness of the shot in the early experiments, and of the accuracy of the method of reduction of those experiments.

But when those coefficients, which have been found correct by the use of the general tables, are employed to calculate trajectories of elongated shot moving with high velocities, the calculated ranges and times of flight gradually fall more and more below those quantities given in the range tables, as the elevation increases beyond 4° or 5°. These defects are generally only small when the variation in the density of the air is taken into account; but their presence indicates some slight disturbing cause independent of the coefficients of resistance. We can now make use of the exact method of calculating trajectories given by modern analysis, which was first published by J. Bernoulli. But this method applies with strictness only to the motion of a spherical projectile, whose centre of gravity coincides with its centre of figure. Many years ago Count St. Robert remarked : "On doit en conclure que les formules ordinaires de la balistique ne peuvent représenter la trajectoire décrite par les projectiles *allongés*" (Balistique, p. 183). Also Mayevski has published an elaborate paper, "De l'Influence du Mouvement de Rotation sur la Trajectoire des Projectiles oblongs dans l'Air" (Technologie Mil., 1866, pp. 1–150), which, however, leads to no useful result beyond showing that the author recognized the effect of drift on the form of the trajectory. The chief cause of the difficulty is this. For a short time after a steady elongated shot has left a rifled gun, the shot preserves the parallelism of its axis, and in consequence of the action of gravity the point of the shot gradually rises above its trajectory till the resistance of the air causes the axis of the shot to begin to describe a conical surface, with nearly constant vertical angle, about the moving tangent to the trajectory. Consequently, soon after a steady elongated shot leaves the muzzle of a rifled gun, the resistance of the air acting on the inclined under side of the shot, begins to raise the shot bodily, and continues to do so until its axis has made one-fourth of a revolution about the tangent to the trajectory. This vertical drift, near the gun, causes the shot to move in its path as if it had been fired at a slightly increased elevation. Consequently, the observed range and time of flight are each somewhat greater than that due to the elevation at which the gun was laid.

Another difficulty, common, however, to both spherical and elongated shot, is caused by the jump of the gun. In the range tables of the 4 and 12-inch guns above considered, six minutes were allowed for the effect of jump for all elevations. But Major Ingalls remarks that "it varies in value from an angle too small to be appreciable, to one of a degree of arc or even more. . . . It also varies somewhat with the angle of elevation." In one of his examples he supposed the jump to be 22 to 23 minutes. Although the jump and vertical drift are uncertain in amount, they have considerable influence on the range and time of flight, and on this account the calculation of trajectories is a decidedly unsatisfactory method of testing the coefficients of resistance of the air to elongated projectiles.

In the early days of elongated projectiles the vertical drift caused by the "kite-like action" of the shot was duly recognized, but of late this disturbing force has been commonly ignored. For now, when a calculated range is shorter than the experimental range, it is at once assumed that the theoretical resistance is too high. This resistance is forthwith reduced so as to make the calculated agree with the experimental ranges, but seldom is any care taken to compare the time of flight calculated with this reduced resistance with the experimental time. It ought, however, to be remembered that, while a reduction of resistance *increases* the range it *diminishes* the time of flight over a given range. Major Ingalls has given a complete example of this method of correcting my resistance (Problems, &c., p. 151). For elevations of

2°, 4°, 6°, 8°, and 10°,

he found it necessary to reduce my coefficients by

4·5 2·3 5·2 8·1 9.7 per cent.,

in order to obtain the experimental ranges by calculation; and these *reduced* resistances gave the calculated times of flight *too short* by

0″·09 0″·12 0″·13 0″·26 and 0″·42,

which proves clearly that the theoretical resistances had been too much reduced throughout. Also, if the method of correction pursued in the above example was correct, it would follow that the coefficient of resistance is a function of the elevation, which is simply absurd.

On the other hand, suppose we correct the elevation so as to make the calculated agree with the experimental range, which seems to me to be a satisfactory *approximate* correction in such cases. A careful calculation of the trajectory of an ogival-headed shot (two diameters) fired from the 4-inch B.L. gun, at an elevation of 15°, gave a range of 6448 yards (185 yards too short by the range table), and time of flight 20″·46 (1″·10 too short), the density of the air having been supposed to vary with the height of the shot. Now, corresponding to an elevation of 14° 16′, the range table gives a range of 6448 yards, and time of flight 20″·53. If we suppose that the elevation of the gun, 14° 16′, was practically increased by 44′ by jump and vertical drift, we obtain an elevation of 15° for the initial direction of the shot. But, according to calculation, for an elevation of 15° we have found the range 6448 yards exactly, and the time of flight 20″·46, which is only −0″·07 in error, and the calculated horizontal striking velocity is 646 f.s. This, I maintain, is the proper method of correction, because it corrects both *range* and *time* of flight, when there is no wind. Using the general tables and the horizontal muzzle velocity, the calculated time over 6448 yards is found to be 20″·53, and horizontal striking velocity 647 f.s., where $\tau = 0{\cdot}967$, the mean density of the air, as the projectile would rise to a height of 1800 feet.

If the above be a correct view of what takes place, it follows that the axis of the shot during its flight is inclined at such a small angle to the direction of its motion, that the resistance of the air to its forward motion is not sensibly greater than when it moves in the direction of its axis. But small as this angle must be, we find evidence of the marked effect of the lateral action of the air in causing the shot to drift to the right towards the end of the range. It is therefore to be expected that the resistance of the air, acting from below on the shot, soon after it leaves the gun will raise the shot upwards, and cause it to move as if it had been fired at an elevation a little greater than that at which the gun was laid.

The Ordnance Committee fired some ogival-headed projectiles from a 9·2-inch wire gun at high elevations in 1888, professedly to try whether calculations of trajectories at very long ranges are trustworthy. But before experimenting they invited calculators to furnish them with the calculated range and time of flight of a 380-pound elongated shot fired at an elevation of 40° with a muzzle velocity of 2360 f.s. My calculations for an ogival head, struck with a radius of one diameter and a half, gave a range of 19,436 yards, and time of flight 62″·15, which were sent in in March 1888. Allowing 300 yards for jump and vertical drift, I obtained a range of 19,736 yards. In April two rounds were fired with a velocity of 2375 f.s. at an elevation of 40°, which gave ranges of 21,048 and 21,358 yards. My reply to this announcement, by return of post, was that the ranges were about 1500 yards too great. In July the experiment was repeated, which gave ranges of 20,236 and 20,210 yards, being a reduction of near 1000 yards. Two rounds fired at an elevation of 30° gave ranges of 17,500 and 18,344 yards; two at 35° gave ranges of 19,420 and 18,963 yards; and one at 45° gave a range of 21,800 yards. The times of flight have not been published. These great variations in range were due in part to unsteadiness in the motion of the shot, but chiefly to the prevalence of high favourable winds.

In order to test guns or coefficients of resistance in a satisfactory manner, *calm weather* is absolutely necessary. And if the shot is expected to rise to a height of two or three miles, trial balloons ought to be sent up to test the state of the currents in the higher regions. Afterwards, when good mean results of experiments have been obtained, then, and not till then, may these mean results be used to test the results of calculation. Experiments ought not to be made at all unless precautions necessary to secure a correct result can be taken.

I have calculated a complete range table for the 9·2-inch gun for elevations 1° to 45°, according to the original programme of the Ordnance Committee (Nature, September 13, 1888, p. 468), where the net results of calculation have been given. These ranges will require an addition perhaps of about 2 per cent. for jump, vertical drift, &c., and a further 1 per cent. if the ogival head be struck with a radius of two diameters instead of one and a half, and the increment of time must be ruled by this increment of range.

My sole object having been to obtain the correct law of resistance of the air to the motion of projectiles, I was always ready to consider any proposed correction. But my results, obtained from numerous and most exact experiments, could be changed only on perfectly satisfactory evidence. That evidence I have failed to obtain in any single case, so that my results remain practically the same as they were given in my original reports, 1868–80. I have recently published a revised account of all my experiments, accompanied by newly calculated general tables, for both English and French measures, and other tables required in the calculation of trajectories, according to the results of modern analysis. With these helps I have now thoroughly tested my final results by the use of range tables of the 4-inch, 6·3-inch, and 12-inch guns, with the most gratifying results. And Major McClintock, R.A., has tested my coefficients for small-arm bullets, with very satisfactory results (Proc. R.A. Inst., xii. 569). This evidence of the accuracy of my results is the more valuable because it is derived from Government experiments, made for other purposes, which have manifestly been carried out with great care and ability. Anyone so disposed has the means to re-examine the whole matter for himself. If there be not some error in my calculations, it appears that my results do not admit of any real improvement, and consequently my labours in this matter may be considered to have reached a satisfactory conclusion.

F. Bashforth.

FORTHCOMING SCIENTIFIC BOOKS.

The following is a list of scientific works which will be issued by various publishers in the course of the spring:—

Messrs. Macmillan and Co.:—"Essays on some Controverted Questions," with a Prologue, by Prof. Huxley; "The Beauties of Nature," by Sir John Lubbock, F.R.S., illustrated; "Island Life; or, The Phenomena and Causes of Insular Faunas and Floras," including a revision and attempted solution of the problem of geological climates, by A. R. Wallace, with illustrations and maps, new and cheaper edition; "The Apodidæ," a morphological study, by Henry M. Bernard, illustrated (Nature Series); "Experimental Evolution," by Henry de Varigny; "The Diseases of Modern Life," by B. W. Richardson, F.R.S., new and cheaper edition; "The Geography of the British Colonies"—"Canada," by George M. Dawson, "Australia and New Zealand," by Alexander Sutherland (Macmillan's Geographical Series); "Scientific Papers," by Oliver Heaviside; "The Algebra of Co-Planar Vectors and Trigonometry," by R. B. Hayward, F.R.S., Assistant Master at Harrow; "Key and Students'